
The Ethics of Deceptive Research Roles Reconsidered: A Response to Kai Erikson

RICHARD A. LEO

The idea of a moral blind alley is a perfectly intelligible one. . . . Given the limitations on human action, it is naive to suppose that there is a solution to every moral problem with which the world can face us.

Thomas Nagel (1979: 74)

I do not believe it is possible to do good fieldwork with deviant subjects and not, morally speaking, get one's hands dirty.

Carl Klockars (1979: 265)

Kai Erikson's "Commentary" in *The American Sociologist* (Summer 1995) strongly condemns the role I assumed in my participant observation research on police interrogators (see Leo, 1995). In Erikson's (1995) characterization of my field work, I "flat out lied," (p.9) engaging "in a degree of deceit that is more widely known in espionage than in social research." (*Ibid.*) I should not "have been there in the first place under those circumstances." (*Ibid.*) Erikson is further troubled by the "casualness in [my] account," (*Ibid.*) which he takes as representative of "an indifference in moral tone that emerges now and then in our professional conversations." (*Ibid.*) If this isn't enough, Erikson charges that I failed to protect my research subjects from invasions of privacy and ultimately spoiled the field for future researchers. For these transgressions—which Erikson believes most sociologists would consider "distasteful and unethical" (p.10)—my dissertation advisers should have objected, my graduate institution should have complained, and the American Sociological Association should have disapproved. Ultimately, Erikson holds out my research as an object lesson "to convey to people who engage in disguised research that the scholarly community to which

Richard Leo is assistant professor of sociology at the University of Colorado-Boulder. Address for correspondence: Department of Sociology, Campus Box 327, Boulder CO 80309-0327.

they belong and in the name of which they conduct research does not approve of that kind of inquiry." (*Ibid.*)

These are strong words, especially because they are written by one of the most well-known sociologists in the world (and a past president of the American Sociological Association) about one of the least known sociologists in the world. Erikson's criticism is not the usual methodological or interpretive disagreement one finds in most academic debates. Rather, it is fundamentally a moral rebuke: "Leo did wrong." (p.10). Such a charge calls into question both my own integrity and the professional judgment of my former advisers. The natural temptation is to respond defensively. Because of the inherently moral character of our debates about field ethics, it is all too easy for professional disagreements to escalate into polemical exchanges and counterattacks. That is not my intention here. While I disagree with much of Erikson's Commentary, I believe that it deserves serious consideration and invites further general discussion about the ethics of deception in field research. As the ethical issues posed by our field studies become increasingly ambiguous, the sociological research community must have open and direct conversations about where we draw our moral boundaries and why.

Whether I "did wrong" and whether my research was "distasteful and unethical" is, of course, a question each reader will have to decide for him or herself. In the brief space of this response, I will not repeat my defense of the research roles I assumed in studying police interrogators (see Leo, 1995), but rather I will highlight my points of disagreement with Erikson. I wish to make five observations:

- (1) Erikson misrepresents the nature of my field role: I did not engage in disguised participant observation. Painting with an unreasonably broad brush, Erikson fails to make crucial distinctions between types and degrees of deception in fieldwork.
- (2) Erikson takes me as representative of a larger phenomenon that he fails to name, for which he fails to provide any references or citations, and about which he offers no substantive information.
- (3) There is no evidence that I invaded the privacy of my research subjects, humiliated, exposed or damaged them.
- (4) I did not argue that "morally reprehensible behavior is OK in professional life if that turns out to be the only way to get access to the data," (p.10) but that fieldwork is a morally ambiguous enterprise. The acceptability of deception varies by its type and degree, by the context in which it is employed, by the roles and relationships we assume, and by the goals and objectives we pursue.
- (5) Erikson's moral absolutism betrays a lack of appreciation for the difficulties of obtaining hidden and dirty data from deviant subjects as well as the potential social value of such knowledge.

First, Erikson misunderstands and thus misrepresents the nature of my research role. Erikson says that I repeatedly, flat-out lied about myself, but this is not an accurate characterization of my field behavior. To lie is to intentionally make statements that are not true. It is an act of commission that involves an

intent to falsify information. That I withheld or concealed information about myself—such as my political views about the death penalty, my sympathy for abortion rights activists and my liberal-minded indifference to homosexuality—should not be confused with the act or intent of falsifying information. Rather, my acts of omission were intended to withhold judgment about, and to express empathy for, my highly defensive and distrustful research subjects, conservative police officers who feared that I was a radical Berkeley graduate student hell-bent to discover and expose police misconduct. Not only does Erikson confuse withholding with falsifying—a crucial distinction—but he says that I did so “repeatedly,” though nowhere in the article Erikson critiques did I make such a statement. Research access comes in stages: once I succeeded in establishing rapport with, and overcoming the distrust of, my various research subjects, they appeared to be far less concerned with my political values and the potential for exposure. And, correspondingly, I felt less guarded in their presence.

The more fundamental issue here is whether, by creating misleading appearances about my political worldview I was, in fact, engaging in disguised research as Erikson suggests. Erikson has long been a leading and persuasive critic of undercover participant observation (see Erikson, 1967). While I share many of Erikson’s ethical concerns about covert research, I did not engage in such covert research. In his indictment of my field ethics, Erikson fails to distinguish between types and degrees of deception. To take an example he raises, there is a world of difference between the research role I assumed and those of the undercover social scientists in Festinger et al.’s (1956) *When Prophecy Fails*. Infiltrating a doomsday cult by concocting elaborate cover stories as new believers, Festinger et al. deliberately lied about their professional identity and deliberately misrepresented the nature and purpose of their research. In effect, Festinger et al. acted as agent provocateurs. By contrast, in my participant observation study of American police interrogators, I came in through the front door—with the Chief’s approval, with introductions from five section Lieutenants, and with the understanding that I would answer all questions about, and accept any necessary limitations upon, my research role. From the very beginning, the Laconia police officers knew my identity as a graduate student and my affiliation with the University of California, Berkeley; they knew the nature and purpose of my participation observation research; and they knew that I planned to write a dissertation and eventually a book from this study. My field research was completely overt. To suggest that I did not identify myself in any useful sense and then to equate this with disguised research, as Erikson does, is simply wrong. It is also unfair.

Second, by singling out my research as representative of a “casualness in . . . account, an indifference in moral tone that emerges now and then in our professional conversations,” (p.9) Erikson’s critique has the effect of scapegoating me for a phenomenon about which I remain entirely unclear. The problem here is not the validity of Erikson’s argument, but rather its vagueness and lack of specificity. Of whom is he speaking? To which professional conversations is he referring? Why are there no references or footnotes in his

account? How often and in what contexts does this "indifference in moral tone" appear? Ironically, Erikson's account here is itself too casual to permit an adequate response. If Erikson is going to ask me to serve as an example of a particular phenomenon in our professional conversations, then it is incumbent upon him at least to provide identifying information about this phenomenon and these conversations. The seriousness of the charges demands more adequate documentation. Again, the reader must decide for him or herself whether my account is casual and my moral tone indifferent. But this seems an ironic comment to make about an article that I wrote precisely because I am concerned about the ethical dilemmas we encounter in our field studies of deviant subjects.

Third, and more specifically, Erikson suggests that I invaded the privacy of my research subjects—humiliating, exposing and damaging them—and that I spoiled the field for future researchers. This is, of course, an empirical question. So far I have received no complaints from my former research subjects, but only from Erikson; at the same time, my former dissertation adviser tells me that the access of his graduate students to the Laconia Police Department has never been better. One way to resolve Erikson's questions might be to ask the subjects themselves. Instead, Erikson simply assumes a positive conclusion to his rhetorical questions: "I don't think Leo would be well advised to go back to the department to find out, and I suspect that none of the rest of us will be welcomed there either. So much for protecting the field for future researchers" (p.9). I am not as troubled by Erikson's conclusion here—which I think is just plain wrong—as I am by the glibness of his tone. Getting inside the interrogation rooms of an urban police department is a difficult task, even with the support of police managers. I see no evidence that in the process of negotiating research access—and creating the appearance of a conservative persona—I invaded my subjects' privacy, humiliated, exposed or damaged them. If any of my actions could have had the unintended effect of spoiling the field for future researchers, it was not my behavior in the field but my decision in the courtroom (months after I had left the field) to comply with a subpoena and testify on behalf of my research subjects (see Leo, 1995).

Fourth, and perhaps more fundamentally, Erikson misconstrues my argument that the standards necessary to carry out the role of a morally competent fieldworker may be different than the moral standards by which we judge human beings in daily life. Contrary to what Erikson asserts, I am not claiming that "we are exempt from the responsibility of acting like morally competent human beings if doing so prevents us from acquiring the kind of data we happen to want" (p.10). Indeed, this would be an absurd position to take. The argument I am advancing is far more subtle and complex: fieldwork is a morally ambiguous enterprise that is fraught with moral hazards, contingencies and uncertainties (see Klockars, 1979). Moral competence varies by context, by the roles and relationships we assume, and by the goals and objectives we pursue. It would be surprising if many sociologists thought differently. The very meaning of moral competence cannot be taken as automatically given: there are few cultural and historical universals in morality (even "thou shalt not kill" admits of exceptions). If the ethics in fieldwork, as in life, are (at least in part)

situational, then it follows that role morality varies (at least in part) by context.

Let me drive this point home by example. Earlier in his commentary Erikson (p.8) tells the following story:

I was consulting for a law firm when I learned that the Sheriff of New Haven was on his way to my front door with a subpoena, giving me just enough time to throw my notes into packing boxes and mail them off to the law firm—where, twenty years later, they still remain, protected by someone else's privilege. I did not have the slightest moral qualm in taking advantage of this shield because it did not seem to me that the attorneys who had sought the subpoena—representatives of a powerful coal company—were engaged in a search for justice.

Leaving aside whether Erikson's views were influenced by his role as an employee of legal adversaries or how he knew that the opposing attorneys were not searching for justice, a good argument could be made that Erikson's behavior—evading a legitimate, court-ordered request for (presumably important) information relevant to a legal dispute—is morally reprehensible, if not in this context then certainly in another. Indeed, Erikson might have considered identical behavior at that time by the expert witness on the other side to be morally reprehensible. For the record, I am not suggesting that Erikson acted improperly. But I ask Erikson: Is it really so astonishing to suggest that a set of behaviors may be morally reprehensible in one context but entirely acceptable in another? Is this really such a surprising argument?

While I am not advocating that we approach ethical issues with an unprincipled moral relativism, I think Erikson's moral absolutism is problematic in theory and unworkable in practice. The ethics of deception in any field—whether medicine, law, policing or social science—raises a number of complicated, vexing, and unsettling questions (See Bok, 1978, 1983). There are no hard and fast rules about what is right and what is wrong across all settings and in all situations. There are no easy answers. There will always be trade-offs, compromises, and competing considerations. Every day each of us makes utilitarian judgments about right and wrong in a wide variety of personal and professional contexts. While ethical judgments must apply both to means as well as to ends, sometimes good ends may justify morally dubious means. This should not be a surprising or controversial argument. The more troubling situation, of course, is when a human dilemma admits of no clear moral solutions but, instead, each possible course of action is associated with a set of moral costs, a problem that is akin to what philosopher Thomas Nagel (1979) refers to as a "moral blind alley" and one that criminologist Carl Klockars (1979) calls "The Dirty Hands Problem." As Klockars notes so well, this is precisely the problem that fieldworkers encounter when studying deviant subjects.

Which brings me to my fifth and final point: Erikson appears neither to appreciate the difficulties of obtaining research data from deviant subjects nor the potential social value of such knowledge. As I have argued elsewhere (Leo, 1994), police interrogation is an intentionally hidden and secret institutional practice about which social scientists and legal scholars know surprisingly

little. While the research problems I encountered may not have been unique to the field study of police (see Van Maanen, 1978) or of deviant subjects more generally (see Klockars and O'Connor, 1979), it is significant that I am the first sociologist ever permitted entree into American police interrogation rooms. Getting there proved to be no small task. I do not believe I could have accomplished this access without assuming a non-threatening persona that allowed me to fit into the world I was studying. This research strategy contained the additional advantage of permitting me to act as unobtrusively as reasonably possible when collecting the data, and thus reducing the likely effect(s) of my presence on the behavior of my research subjects.

Needless to say, I believe that my field research will yield important knowledge that is valuable both for its own sake as well as for its policy applications. Regardless of the impact of my study, however, we are not going to acquire much "hidden and dirty data" by always wearing white gloves (See Marx, 1984). Minor uses of deception may be necessary simply to obtain such data; they may be justifiable when the harm caused to our subjects is slight or inconsequential and the social and intellectual value of the knowledge is considerable. Indeed, the ethical codes of the American Sociological Association, the American Psychological Association and the American Anthropological Association all permit minor uses of deception in social science research. Erikson's categorical imperatives, however, altogether foreclose this possibility. Woodward and Bernstein's investigations would never have led to Watergate if they had followed Erikson's absolutist ethics. To take an example closer to home, sociologist Martha Huggins has been able to obtain interview data in her study of the social psychology and discourse of former Brazilian police torturers only by leading her research subjects initially to believe that she was conducting a study of policemen in times of crisis and conflict (Huggins and Haritos-Fatouros, 1994). I suspect that few sociologists would object to Huggins's benign deceptions.

To conclude, in his Commentary Kai Erikson misrepresents the nature of my field role, advances sweeping accusations that lack clear referents, and misconstrues my arguments about the ethics of deception in the study of deviant subjects. There is no evidence that my role behavior inside police interrogation rooms harmed my research subjects. Nevertheless, Erikson calls attention to a number of important ethical issues that should be of interest to all research sociologists. Underlying both Erikson's critique and my response is a fundamental concern with the permissibility and limits of deception in social science fieldwork. While I believe these issues are more complicated and ambiguous than Erikson allows, both papers undoubtedly raise far more questions than they answer. These ethical issues deserve more attention than they have thus far received by sociologists, especially as courts increasingly attempt to coerce confidential testimony and research notes under threat of indefinite imprisonment (see Leo, 1995; Scarce, 1994). If sociology is to be held accountable as a profession, we must openly and directly confront the complicated moral issues raised by the use of deception in qualitative field studies, despite (if not because of) our vast differences of opinion.

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